Correspondence

First, do no harm

I welcomed the special article by Bailey et al. I share the authors’ concern over the ‘scandal of premature mortality’ and note their recommendation to urgently review antipsychotic medication when certain adverse effects are experienced (rapid early weight gain or cardiometabolic blood disturbance). The authors do not implicate any particular antipsychotics, but guidelines suggest that clozapine and olanzapine are the most likely antipsychotics to be associated with these side-effects. Neither do the authors suggest what the outcome of such a review might be, although I deduce it is implicit in the recommendation that reducing the dose or switching antipsychotic would be likely possible outcomes. I do, however, have one concern with this suggestion which relates to the risk-benefit balance of antipsychotics.

Tiihonen et al1 present data from a large study which examined the effects of antipsychotics on all-cause mortality, suicide and deaths from ischaemic heart disease; one strength of this study is the examination of all-cause mortality. The researchers found that in people with schizophrenia antipsychotic use is associated with a reduced risk of death (by about a third) when compared with no antipsychotic treatment (hazard ratio 0.68, 95% confidence interval 0.65–0.71); clozapine was associated with a substantially lower risk of all-cause mortality as well as suicide. No pronounced differences between antipsychotics (including clozapine and olanzapine) were noted for mortality from ischaemic heart disease.

Thus, if a patient is switched from clozapine to an alternative antipsychotic, their risk of death may in fact be increased rather than reduced. Further, switching antipsychotics (even olanzapine) does not appear to be associated with a reduction in risk of all-cause mortality or even death from ischaemic heart disease. Given that switching antipsychotic medication is associated with harm, for example by increasing risk of relapse,4 this leads me to question the wisdom of Bailey et al’s recommendation to urgently review the antipsychotic prescription in the circumstances they describe.

There may be other reasons for switching antipsychotics but Tiihonen et al’s findings suggest that reducing the ‘scandal of premature mortality’ is not one of them. This raises a dilemma for practising clinicians as to how to proceed in these circumstances.


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Cardiovascular disease and schizophrenia: do we know enough?

We find the aims of Bailey et al’s laudable. However, we would like to add a note of caution. Our main concern is that many of the recommendations are not based on evidence. Bailey et al assume that people with schizophrenia are the same as the general population, the so-called ‘ecological fallacy’. The authors describe potential differences such as the increased risk of metabolic abnormalities including diabetes which pre-date the prescription of antipsychotics. Therefore, it cannot be assumed that what is effective in the general population will be equally effective in people with schizophrenia. For example, controversy surrounds the diabetogenic effect of statins in the general population and Nielsen et al demonstrated that lipid-lowering medication was a greater risk factor for the development of diabetes in a cohort of people with schizophrenia than was ‘high-risk’ antipsychotic medication. Furthermore, a Finnish cohort study replicated the finding of poor outcomes for cardiovascular disorders in patients with schizophrenia and reiterated that the excess morbidity could not be explained by prescription rates of lipid-lowering drugs.

Bailey et al present a comprehensive overview of cardiovascular risk management and although we may be guilty of the same assumption as the authors, we would like to emphasise the importance of cardiorespiratory fitness as a modifiable risk factor. Its significance is often neglected or understated, with guidelines emphasising medical management. However, Kilbourne et al reported that physical inactivity (hazard ratio 1.66, 95% CI 1.59–1.74) was a greater risk factor than smoking (hazard ratio 1.32, 95% CI 1.26–1.39) for cardiovascular mortality in a cohort of people with schizophrenia. The complexity of mortality risk factors in early schizophrenia is further illustrated when one examines the relationship between body mass index (BMI) and suicide in the general population. Suicide, and not cardiovascular disease, is the major mortality risk in younger people with schizophrenia. An emerging paradox is linking an inverse association between BMI and suicide risk in the general population; hence a lower BMI may reduce cardiovascular risk but increase suicide risk. Whereas there is emerging evidence that patients with schizophrenia are receiving medical treatment for cardiovascular risk factors, there is little evidence so far that this has reduced mortality.1

If the people with schizophrenia are seen as a high cardiovascular risk population with attendant early and aggressive medical intervention, the impact on core symptom outcomes needs to be studied as some of the antipsychotics with the greatest liability for metabolic side-effects are also the more effective. Clearly, more research is required to understand the relative importance of mortality risk factors in schizophrenia and their management.5

Declaration of interest

R.E.H. and M.B. have received research funding and hospitality from pharmaceutical companies. H.W. is an ex-Lilly employee.
Iatrogenicity: are we largely to blame for this epidemic?

Notwithstanding the premorbid genetic and psychosocial predispositions Bailey et al refer to,1 the authors also correctly highlight the incontrovertible evidence that the obesity and metabolic syndrome epidemic we are facing is largely drug induced, as highlighted by the EUFEST study.2 Given this, we must accept that we are essentially complicit in greatly increasing our own patients’ morbidity and mortality, and that this ‘epidemic within an epidemic’ is iatrogenic. I cannot help but wonder whether we, as clinicians, tend to ignore a side-effect which we consider to be ‘benign’, in relation to the perceived lack of an immediate need to address it urgently, as opposed to, for example, an acute extrapyramidal side-effect, massively raised prolactin or marked electrocardiogram changes. I wonder whether our complacency in addressing this adverse effect profile may be borne out of a sense of our own helplessness. That is to say, because there is no straightforward solution to this multifaceted problem, we choose to ignore or at least sidestep the issue. It is precisely because of the creeping, insidious nature of these obesity-related problems that we are allowing them to develop into an ‘epidemic’ of such proportions.

We must ask ourselves whether it is morally acceptable to treat chronic and enduring mental illness at the expense of inflicting chronic and enduring physical illnesses. As the authors allude, if we actually bothered to ask our patients, particularly the younger ones, what it is they would be most distressed by – continued mental illness or aggressive weight gain – would it really be so surprising that a sizeable proportion would prefer to remain distressed by (or learn to cope with) their psychiatric symptoms than become morbidly obese? Should this really come as a shock to us, given the strongly body-conscious world in which we live? I suspect that our priorities as psychiatrists may not be entirely aligned with those of many of our patients. Is there a doctor–patient risk–benefit analysis mismatch at play here?

But are we really improving our patients’ quality of life and promoting social inclusion by treating one stigmatising condition for another, which arguably carries even greater prejudice? After all, most of the population view morbidly obese people not only as a repulsive eyesore, but tend to apportion blame. Many view obesity as a self-inflicted condition, borne purely out of laziness and gluttony, and tend to make extremely pejorative judgements.

Notwithstanding this, although antipsychotics are the only truly effective weapons in our armament against chronic psychotic disorders, it is incumbent on us to make prescribing decisions which take from the outset the potential ramifications of such physically and socially disabling adverse effects into account.

At the end of the day, if I was a patient, I would not be happy to learn that I had developed a serious, chronic physical disorder with many potential multisystem complications (such as diabetes) as a result of taking a drug which I probably was not keen to take in the first place anyway, and was never fully appraised of the risks. We must never be economical with the truth about the drugs we are all too happy to dish out.

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Physical health epidemic in mental health

We would very much welcome the focus on physical health from secondary mental health, as advocated by Bailey et al.1 However, we would like to raise the following points.

The Quality Outcomes Framework2 now includes HbA1c levels recorded in the past 15 months to identify diabetes for patients aged 40 years and over with schizophrenia, bipolar affective disorder and other psychoses (MH20). It is worth noting that the World Health Organization has included HbA1c in its diagnostic criteria for diabetes and this is also being backed up by the National Institute for Health and Clinical Excellence.3 We think that it is important to have HbA1c levels recorded, especially in patients on antipsychotics.

The incidence of metabolic syndrome in psychiatric patients has been covered recently in this journal,4,5 but Bailey et al could have highlighted the need for baseline physical health monitoring before commencing on antipsychotics. Moreover, there is a known higher incidence of diabetes in patients with psychosis. Therefore, psychiatrists play a major role in reminding other clinicians and reiterating in their communication to general practitioners the importance of following parameters such as weight, blood pressure and glucose levels in the early weeks, so the primary care team are aware and the patients are appropriately followed up and supported.

Bailey et al seem to be suggesting that antipsychotics have no role in the management of psychosis and the disorder can be treated with a multiprofessional approach. It might have been better to mention the impact of duration of untreated psychosis on the long-term patient-related outcomes,6 and so I would have thought that antipsychotics would be the essential

References


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part of a biopsychosocial approach rather than a treatment of last resort. Finally, I am glad to hear about the Royal College of General Practitioners’ involvement with the Royal College of Psychiatrists in coming up with a collaborative framework. I welcome the Bailey et al article and the joint collaboration and would hope more joint work is carried out in the future between primary and secondary care teams.


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Authors' response: Dr Chaparala asks if we would have been better mentioning how duration of untreated psychosis affects long-term outcomes. Is not a 20-year mortality gap for men, and 15 years for women, a significant long-term outcome and an impact of untreated cardiometabolic risk deserving of some earlier intervention?

Notwithstanding incontrovertible evidence that antipsychotics cause problematic weight gain, we do not suggest antipsychotics are the sole explanation of increased cardiovascular disease, but do highlight how antecedent risks can become established in the critical early treatment phase. This is further supported by another recent systematic review observing cardiometabolic changes only after antipsychotic initiation. The subsequent trajectory of weight gain, increasing metabolic disturbance and sustained heavy smoking provides a compelling link between schizophrenia and cardiovascular disease, the single most important cause of premature death in this population.

Furthermore, the National Institute for Health and Clinical Excellence (NICE) are clear in their recommendations that these adverse cardiovascular risks should be identified at the earliest opportunity and managed using the appropriate NICE guidance for prevention of these conditions (the 2009 updated guidance for schizophrenia, CG82; recommendation 10.4.1.3). And yet when the recent Royal College of Psychiatrists’ National Audit of Schizophrenia (NAS) examined the implementation of NICE recommendations in community settings (NAS report 2012; www.rcpsych.ac.uk/quality/NAS), it found that only 29% of people with schizophrenia across England and Wales had received an adequate assessment of cardiometabolic risk within the previous 12 months; 44% had not even been weighed.

Does this apparent lack of concern about adverse cardiometabolic consequences revealed by the NAS matter? After all, Dr Reed is reassured about antipsychotic safety by the FIN11 study of Tilhonen et al. However, authorities De Hert et al have challenged this study’s conclusions, listing methodological weaknesses which include ‘incomplete reporting of data, questionable selection of drug groups and comparisons, important unmeasured risk factors, inadequate control for potentially confounding variables, exclusion of deaths occurring during hospitalization leading to exclusion of 64% of deaths on current antipsychotics from the analysis, and survivorship bias due to strong and systematic differences in illness duration across the treatment groups.’

Dr Reed raises the issue of switching antipsychotics and how this may destabilise control of psychosis but may have missed the point of Weiden’s editorial that he refers to. While indeed not advocating switching antipsychotics in someone established on treatment, Weiden highlights how two randomised studies demonstrated the positive value of switching antipsychotics to counteract rapid weight gain and metabolic change, concluding: ‘Practice guidelines and public policy should recommend that clinicians consider the value of switching antipsychotics in patients with elevated metabolic risk.’

Dr Chaparala suggests we are abandoning antipsychotics. No, but we are in good company in questioning the dominance of psychopharmacology. Moreover, excessive reliance on antipsychotic treatment is suggested by the NAS finding of wide variation in the availability of psychological treatments across England and Wales: even in those patients whose response to antipsychotics had been unsatisfactory, 34% were not offered any form of psychological treatment despite NICE recommendations that these should be considered.

What we urge is responsible prescribing, particularly in the critical early phase of illness and sensitivity by us as doctors to how these young people may feel about the effects of our treatments. Perhaps the final word should go to the closing comment of Dr Tagore’s letter: ‘We must never be economical with the truth about the drugs we are all too happy to dish out.’

Declaration of interest D.S. is current member of two Guideline Development Groups (GDG) for NICE: NICE guidance for children and young people affected by psychosis and schizophrenia, and NICE guidance for adults with psychosis and schizophrenia. The views expressed are not those of GDG, NCCMH or NICE. (The declaration applies to this letter and to the original article.)

3 De Hert M, Correll CU, Cohen D. Do antipsychotic medications reduce or increase mortality in schizophrenia? A critical appraisal of the FIN-11 study. Schizophr Res 2010; 117: 68–74.
A case of clozapine-induced diabetic ketoacidosis

A 29-year-old male of Yemeni descent detained in a medium secure unit was commenced on clozapine; after 4 weeks of treatment he was taking a total of 275 mg in divided doses. He developed nausea and vomiting which progressed over 36 hours to a point where he needed to be urgently transferred to the local accident and emergency unit. At assessment he was experiencing breathing problems, vomiting and he was incontinent of urine; he had a Glasgow Coma Scale score of five. He was immediately transferred to the intensive care unit. The differential diagnoses included drug overdose, alcohol intoxication and clozapine-induced hyperglycaemia. His blood chemistry showed evidence of diabetic ketoacidosis; his blood glucose level was grossly elevated. The clozapine was stopped and the patient was given appropriate treatment with glycaemic agents.

In summary, the patient had become seriously unwell over a period of 36 hours. Apart from having a slightly raised body mass index, he was fit and well and had no family history of diabetes. His pre-treatment blood glucose had been normal.

Diabetic ketoacidosis is over ten times more common in patients treated with atypical antipsychotics than in the general population, although the evidence is largely restricted to case reports and series. Clozapine has a higher risk of ketoacidosis than other oral antipsychotics and it tends to develop after a shorter duration of treatment, with a high proportion of patients developing it within 3–6 months. Low doses, being a young male and having a negative family history to case reports and series. Clozapine has a higher risk of mortality. There is also significant mortality. The unusual aspect of this case (although not unknown) was the occurrence of diabetic ketoacidosis during the titration phase of treatment.


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7 Professor Sue Bailey, MBChB, FRCPsych, President of the Royal College of Psychiatrists, consultant child and adolescent forensic psychiatrist, Greater Manchester West Mental Health NHS Foundation Trust, and Professor of Child and Adolescent Mental Health, University of Central Lancashire.
8 Dr Clare Gerada, FRCP, FRCGP, FRCPsych, Chair of Council, Royal College of General Practitioners, and GP partner, Hurley Clinic, London.
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‘Deaf-mute’: time to abandon stigmatisation of the deaf community

I was dismayed to read Akintomide et al’s reference to the subject of their case review as a person who was ‘profoundly deaf-mute’.

‘Deaf-mute’ is an outdated term originating in the 18th/19th century. It carries very derogatory connotations, and is no longer used in reference to individuals with profound deafness. The term ‘mute’ implies a lack of ability to make noise. Such a label is technically inaccurate when applied to deaf individuals, since they generally have functioning vocal chords and therefore retain the ability to make vocalisations (http://wdeaf.org). Those who are profoundly deaf from early life struggle to develop an oral language, given that hearing is required to facilitate a modulation of one’s voice into speech. Many will therefore employ non-verbal communication in the form of sign language instead. This is a complex combination of hand signals, with its own regional dialects and international differences.

Over 75,000 people in Britain currently use British Sign Language (BSL) as their first or preferred language. The majority of these sign language users consider themselves as members of a distinct cultural community with a strong social identity.

To this day the social image of deafness remains impaired on an international scale. This manifests itself in the form of a deeply rooted pathological stigma, negative stereotypes and prejudiced attitudes towards the deaf. It would seem that such ignorance also persists among health professionals. Ralston et al surveyed the attitudes of 165 physicians and identified a significant difference in attitudes towards hearing patients compared with deaf patients. Munoz-Baell & Ruiz suggest that much of the stigma relating to the deaf community arises from an extensive social lack of appreciation of both their communication mechanisms and their culture. Unfortunately, in spite of more recent advances in healthcare legislation, it would appear that there is still some way to go before members of the deaf community achieve the equality of health and social standing to which they are entitled.

The summary for Akintomide et al’s paper states that it is the first published case report of catatonia in someone who is profoundly deaf. It is a shame therefore that, rather than taking the opportunity to present a positive reflection of managing patients with profound deafness, the authors have merely succeeded in perpetuating existing negative stereotypes about this sector of the population.

N. Deaf is used in reference to those born deaf whose first language is BSL. It is used as a generic term, and for those with acquired deafness whose primary form of communication is oral.

The impact of the Health and Social Care Act 2012 on forensic psychiatry

As a National Health Service forensic psychiatrist working on a newly commissioned low secure ward, the statement: ‘it is all too predictable that yet more patients will be pushed down forensic care pathways from which return to mainstream care will be difficult (p. 402)’ in Holloway’s excellent November editorial1 struck a firm chord with me.

In the past year, I have overseen an expansion of both the low secure forensic estate and the out of area patient placements. Although there was some clinical and commissioning intent to introduce the low secure estate to allow transition out of the medium secure estate (and indeed this has happened to some extent), there has been quite a surge of patients coming from the general acute services and the community.

We also receive some prison transfers; these include general adult community patients with no prior forensic history who were missed in the community owing to (poorly resourced) service lapses. Such patients become ‘forensic’ because of a lack of adequate community psychiatric services rather than being appropriate referrals to the service. In any case, we are expanding.

Good news for forensic staff, but not so good for patient care. Earlier psychiatric intervention for them may have even saved them from being locked up in prison. This is low-income country psychiatry in a high-income country.

At a recent presentation by some Californian psychiatrists, I was very impressed by the vigour with which they grapple with often very difficult legal circumstances of psychiatric care in their jurisdiction. They noted that most of their state hospital beds were occupied by their forensic patients. There was very little available for non-forensic patients, either in hospital or in the community. I wonder whether here in England we are also heading in that direction.

Finally, it appears that in this evolving, risk-focused, forensic-heavy psychiatric care environment, the ‘forensic’ patient today is not the same forensic patient from 20 years ago. These days, not every forensic patient is a high secure step-down patient. Why is it then more difficult to discharge forensic patients into the community, and return them to mainstream services? At the very least, the expanding low secure estate ought to provide an easier interface within the psychiatric services than was the case in the past. This way we will have done our best for our patients while contending with the difficult care environment being planned for us by this government. Indeed, who else will?

Having attended a local third-sector and service user conference and having read the editorial by Holloway,1 I wonder whether the following needs more consideration.

It strikes me that dividing mental health commissioning responsibilities locally between the clinical commissioning groups (mental illness treatments) and local authorities (suicide and substance misuse prevention, mental health promotion) poses unnecessary complexity and bureaucratic waste. Despite lay representation in clinical commissioning groups, there is no democratic accountability similar to that offered through local councillors and local authority scrutiny committees which can call providers to attend a public meeting to account for their priorities in using public funds. Perhaps local elections might be more popular if electors realise that councillors could be voted out if they are not active in championing mental health issues such as dementia care. Furthermore, local authorities already have experienced procurement teams with ready access to performance management and audit functions.

Therefore, I wonder whether clinical commissioning groups should be relieved of all mental health commissioning responsibilities, with this function carried out entirely by local authorities. This would allow the commissioning groups to concentrate on acute and chronic medical diseases (which contribute to most of the cost via hospital bed usage and new technology). The added benefit of mental health being commissioned by local authorities would be integration of social and healthcare budgets for the benefit of people with severe mental illness such as psychosis and dementia. As a practising clinician, I find it difficult to separate social and health interventions in providing a good outcome for an individual patient; usually, there is a synergistic effect.

The other issue discussed by Holloway is ‘personalisation’. It is hoped that by April 2013, 70% of eligible mental health service users (mainly with severe chronic illness) will have a personal budget with an allocated broker to help clarify and achieve their choices in interventions. The above rationalising of commissioning would lend itself to a combined health and social care budget which can be spent pragmatically. A chip-and-pin charge card could be introduced to carry a combined budget, with greater accountability and freedom from having to collect receipts.

The third issue highlighted at the conference was an increasing body of evidence suggesting that active collaborations between statutory mental health providers and third-sector organisations result in better outcomes and lower number of bed days in psychiatric hospitals. Perhaps this should be considered an essential requirement for mental health trusts when submitting bids for a service.
Specialist community teams backed by years of quality research

In response to Dr Killaspy’s invited commentary on Dr Lodge’s piece favouring generalist vs. specialist mental health teams, professor Burns laments that ‘every change, no matter how hare-brained, is hailed “an innovation”’. He implies that it is ‘hare-brained’ to implement crisis response, early intervention and assertive community treatment (ACT) specialist teams, even though they all have unambiguously strong international evidence of both persistent effectiveness and economic advantage (e.g. Killaspy & Rosen5). We share Dr Lodge’s key concerns for continuity of care and the need to engage some individuals in long-term therapeutic relationships. For instance, ACT and early intervention psychosis (EIP) teams are specifically designed to amplify these functions, for those who need them and only while still needed. This has been readily addressed by having a generic front-end community mental health team (CMHT) co-located with primary care where possible and specialised back streams. This results in mutually supportive and often shared working between all these teams. Transfers, where they occur, are very slow, so continuity is preserved. Professor Burns and Dr Lodge argue from a false premise, as pitting generic against specialised teams is a ‘straw-man’ argument. They provide no evidence in support of retaining the generic status quo alone, just moral assertions. The status quo is often hailed as the ‘tried and tested’ condition to beat, when ‘there is surprisingly little evidence to show that [CMHTs alone] are an effective way of organising [community] services’, as stated in the National Institute for Health and Clinical Excellence guidance on managing schizophrenia in adults (CG82, p. 336).

Professor Burns accuses Dr Killaspy of being ungenerous, unjustified and disingenuous for standing up for systematised team approaches that have strong evidence internationally, in comparison with our more habitual comfort as clinicians with undifferentiated CMHTs and more traditional, hospital-centric and sedentary out-patient care. ‘Newer is not necessarily better’ he posits. Well, we appreciate his clinical conservatism. But, in stating that ‘Nobody waits to see if it makes any difference, never mind delivers an improvement’, how long does he wish us to wait, while depriving severely disabled UK citizens of an effective service delivery system (ACT) which has just been celebrated for more than 40 years since initial high-quality randomised controlled trials proved strongly favourable and cost-effective (e.g. studies by Stein, Test and Westwood), with waves of positive international replications since?

Over recent years, professor Burns and colleagues have muddied the waters by implying that indifferent results for even more diluted models of ‘intensive case management’ in the UK such as the UK700 and PRiSM studies somehow represented ACT, and proved that it did not provide any advantage in UK or Europe over CMHTs. They deem ACT to be unnecessary where, in comparison with other countries, there is an adequate health and Social Services ‘safety net’. Yet its effectiveness in Australia and Canada has been demonstrated in the context of a public health and welfare system at least as good as the UK’s at its best. Meanwhile, these much-vaunted ‘safety nets’ are now unravelling in many parts of Europe. This misleading position adopted by Burns and colleagues must bear some responsibility for this premature disinvestment, for the further dilution of these teams under financial pressure, and for the dampened enthusiasm for the UK research effort into ACT, when it has only just begun, with mixed results possibly owing to patchy team fidelity.4

Tragically, severely and persistently mentally ill Britons will suffer with neglect because of the partial dismantling or withdrawal of these essential integrative community care delivery systems. Community-based teams in the UK need their capacity to consistently follow the fidelity protocols of these specialist teams upgraded, not dismantled. This is a challenge to rigorous science, to sound commissioning, to communal action and ultimately to good government.

Declaration of interest

R.D. is editor of a consumer-oriented newsletter sponsored by Johnson & Johnson.

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3 Burns T. Newer is not automatically better (e-letter). Psychiatrist 2012; 22 October.

A full list of references is available in an online version of this letter.

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